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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Implications from palaeoseismological investigations at the Markgrafneusiedl Fault (Vienna Basin, Austria) for seismic hazard assessment" *by* Esther Hintersberger et al.

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The paper presents the analysis of three trenches studied along the Markgrafneusiedl fault, a normal fault associated to the Vienna Basin Transfer Fault. Slip history and timing of paleo ruptures is performed through a paleosiemsological analysis incorporating OSL and IRSL dating.

I think the paper could be suitable for NHESS after moderate revisions are undertaken. Evidence of paleo-earthquake are based on cumulative displacements and on

the presence of colluvial wedges. These wedges should be better described and discussed; gelifluction processes seem, in my opinion, could be the responsible of the wedge-shaped geometry of some of them (which do not necessary are tectonic colluvial wedges).

Given the implications on seismic hazards, three clue issues should be better discussed: 1) periodic vs clustered behavior; 2) primary vs secondary ruptures; 3) relation of EQ chronology with glacial retreat.

I encourage the authors to address these points in order to turn this manuscript in an improved presentation and discussion of their excellent findings.

Please fill free to contact me if my comments are not clear,

Sincerely

Maria Ortuño (Univ. of Barcelona)

13th July 2017 Paleoseismological data:

-Well and comprehensive presented paleo-seismological data. However, as a paper reporting paleoseismological data, I miss:

1. A general sketch of the sites, with the geomorphological features is missing. 2. Some pictures of the landscape (at least one) would help the reader to understand better the setting. 3. The logs (Fig. 4, 6 and 8) need to show subunits discussed in the text. Photologs as supplementary material would help a better understanding of the descriptions. Often, deformational features are referred in the text but cannot be identified in the logs or in the pictures. Make sure you indicate/locate them. In general, I miss more references to the already provided pictures of the trench walls (Fig. 5 and 7). 4. Event horizons should be included in the logs as lines, or at least points. Only letters are insufficient to exactly locate the stratigraphic position of the event. Within the text, the events should be described as defined by bracketing units (upper/lower). That is a unit-constrain of events, usually present in paleoseismic studies. This is different

than the time constrain provided in section 5. If in the future, units are re-dated (which is quite common), the definition of events can be kept, still is valid. 5. The internal structure of the sections describing the trenches should be parallel. For instance, colluvial wedges in SDF1 are described together with the stratigraphic unit, but in SDF3 they are not mentioned until the section discussing the events. 6. “deformation bands”. I might be wrong, but in most papers dealing with fault exposure, those bands are simply called fault zone banding of foliation. 7. The section analyzing the events in trench WAG is missing. Expected section 3.3.1.

There are two parts of the text where I suggest alternative interpretations to the deformational features observed in trench SDF1 (where colluvial wedges should be discussed better. The role of geli-fluxion in the formation of “tailed wedges” should be considered, in my opinion.) and in trench WAG (where the structure of “deformational bands” resembles a fold-scarp, not a foliated fault zone). See specific comments to these issues below.

The trenches always show very distinctive materials in the hanging compared to the footwall. Do you think the faults controlled the sedimentation in the hanginwall in most cases? o.e., they acted as physical barriers hampering the sedimentation in the footwall (with some exceptions). I think it is interesting to briefly discuss this subject.

Dating results.

-This part is quite methodological, but I found it interesting. Perhaps, this could be included as an appendex, because it “breaks the flow” of the manuscript. I would only leave in the main text the Dating results.

Paleoseismological discussion.

I found four main problems/issues (commented below in the comments to sections): 1) The definition of the events (particular and common events) should be first done in terms of bracketing units. Then, age constrains can be exposed (based on limited

number of samples, as usually). Figure 10 should be improved. 2) Maximum expected magnitudes. I suggest to compare values with those derived for surface ruptures, and move all those reference to section 6. In the last years, the Mmax derived from observed displacement is not well accepted (as far as I understand, I give some references). Event displacement might be highly variable along the trace. Wells and Coopersmith might not be representative for slow faults in continental settings. 3) I have done some comments in the Discussion of the periodic/aperiodic behavior (see below). I don’t think any of the two proposed scenarios lead to infer periodic behavior. 4) The possibility of the MF being a secondary fault of the VBTF should be discussed. It is quite relevant for hazard estimates to consider these two as primary Eqs sources. Is this paper providing robust data in a primary seismogenic nature of the MF?

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-126/nhess-2017-126-RC4-supplement.pdf

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